

Job Lock, Retirement, and Dependent Health Insurance: Evidence from the Affordable Care Act *

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Abstract

The 2010 Affordable Care Act expanded health insurance coverage to dependents up to age 26, allowing some parents to add adult children to their employer-sponsored plans. I leverage this policy to understand the role adult children play in their parents' labor supply, and consider a potential spillover of the dependent mandate policy to parents: did parents delay retirement to take advantage of the policy? I find that among parents aged 55-66, affected parents' retirement rate fell by 3.8 percentage points after policy enactment, causing them to delay retirement by 0.74 years on average. An estimated 290,000 parents delayed retirement in order to obtain coverage for their children.

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1 Introduction

One of the most important labor supply decisions an individual makes in her lifetime is the decision of when to retire. As pension funds and Social Security programs have become increasingly strained by greying populations in recent decades, the need to understand when and why workers retire has only grown in importance. The retirement decision is a complicated, multi-dimensional one, which explains the extensive literature devoted to understanding it. Much of this literature has focused on studying the individual and spousal factors that determine retirement timing, such as financial incentives, health, and own/spouse health insurance availability.¹ But one potentially important factor has been mostly overlooked: children. In this paper, I provide causal evidence that parents delay retirement in order to obtain insurance coverage for their adult children. This highlights the important role that adult children play in their parents' retirement decision, demonstrating how policies aimed at "children" can have spillover effects on their parents' behavior even into adulthood.

Perhaps one reason why the role of children in the retirement decision is understudied is that for many parents nearing retirement age, their "children" are actually adults who are no longer technically part of the household. So while there is a substantial literature documenting that these children were a major factor in their parents' labor supply when they were young (e.g., Angrist and Evans 1998; Kleven et al. 2018; Bertrand et al. 2010), once these children are adults, they may no longer play a role in their parents' career decisions. However, there is also growing evidence that parents still actively support their children even into adulthood. In the U.S., 53 percent of young adults reported receiving some form of financial assistance from their parents (Country Financial 2018), and it is estimated that parents give their children a total of \$50,000 in inter-vivos transfers from age 53 on, excluding bequests (Hurd et al. 2011). There is already some survey evidence that this support affects parental retirement in the context of college tuition.² Thus, as many parents seem to financially provide for their children even into adulthood, the role that adult children play in their parents' retirement decision merits its own investigation.

¹Coile 2004 provides a comprehensive overview of the literature on factors that contribute to the retirement decision.

²In a survey of 2,015 students and parents, almost a third of parents stated that they may have to delay retirement to pay for their child's college tuition (Business Wire 2019). It has also been found that parents currently paying for college tuition are less likely to retire (Handwerker 2011).

This paper provides evidence that adult children indeed continue to play a major role in determining their parents’ labor supply. I show how a policy aimed at expanding insurance coverage for adult children by tying it to their parents’ insurance had spillover effects on subsequent parental retirement behavior. While previous studies have demonstrated that health insurance benefits can distort labor supply and generate so-called “job lock” (Madrian et al. 1994; Gruber and Madrian 1995; Gruber and Madrian 1997; Dave et al. 2015; Garthwaite et al. 2014), it is not clear how much of the distortion stems from *own* as opposed to *dependent* coverage, as the two are often packaged together. Using a policy which only expanded dependent coverage, I show that dependent insurance itself generates substantial job lock for parents in that it prevents parents from retiring when they otherwise would have. In fact, it can distort parental retirement plans even when the dependents in question are adults who mostly live out of the household and may have access to alternative sources of insurance.

The policy I consider is the Affordable Care Act (ACA) dependent mandate, which required insurance plans covering dependents to cover them until age 26. Comparing retirement rates of parents affected by the policy and parents whose children were too old to qualify, I find that those whose children gained coverage were 3.8 percentage points (18.2 percent) *less likely to retire*. As health insurance is tied to work for most Americans under 65,³ I interpret this as evidence of parental job lock induced by the expansion of dependent coverage for young adults.⁴ The reduction in retirement probability translates into an average retirement delay of 0.74 years. Extrapolating this to the entire U.S. population implies that about 290,000 parents delayed retirement in response to the dependent mandate.⁵

I check the robustness of this result through a series of placebo tests and robustness checks. I consider the retirement rates of the two groups *before* policy implementation, retirement rates of two unaffected groups, and the effects of a placebo “policy” using an earlier time period. All of the placebo tests are insignificant. The findings are also robust to alternate specifications that change the definition of the treated and control groups.

³In 2016, the majority of Americans (55.7 percent) were covered by employer-based health insurance (Barnett and Berchick 2017).

⁴Indeed, parental employer-sponsored insurance coverage rose by 30 percent among young adults aged 19-25 after the ACA (Antwi et al. 2013).

⁵In comparison, Antwi et al. (2013) found that about 2 million young adults added parental employer-sponsored insurance after the ACA passed.

The mandate’s eligibility cutoff age of 26 for dependents allows for a clean separation between affected and unaffected parents. If we assume that having a child slightly older or younger than 26 in 2010 is as good as random, then what I estimate is the causal effect of dependent health insurance on parental retirement. The policy context of the ACA mandate enables me to isolate the effect of *adult* children, since the mandate effectively expanded coverage only for adult dependents. It did not change coverage for individuals, spouses, or any dependents 18 or under. The ACA mandate also allows me to circumvent another practical challenge to studying this relationship: there is a relative lack of data sources which include both parents and adult children living *out* of the household and which also have sufficient sample size to support many identification strategies.⁶ To execute my identification strategy with the ACA mandate, I simply need to know the child’s age in order determine eligibility. Unfortunately, the Current Population Survey and the American Community Survey only ask about the year of first birth to women aged 15-50, which would exclude most retirement-age parents. This information is, however, available for parents of all ages in the Survey of Income and Program Participation (SIPP) dataset, which is the dataset I use.

This paper also contributes to a growing literature documenting the effect of the ACA dependent mandate, which has mostly focused on how it affected young adults.⁷ The mandate increased young adults’ insurance coverage (Antwi et al. 2013), improved health in a variety of dimensions (Barbaresco et al. 2015; Robbins et al. 2015), reduced healthcare debt (Blascak and Mikhed 2018), and increased wages (Dillender 2014). The evidence on job lock for young adults is mixed, with some papers finding a decreased probability of working full time and hours worked (Antwi et al. 2013), but others finding no effect on their job mobility and employment (Bailey and Chorniy 2015; Heim et al. 2017). In contrast, my paper finds substantial job lock for another party affected by the dependent mandate – the parents who, by staying at their job instead of retiring, are providing the insurance.

Overall, my findings suggest that adult children are an important factor in their

⁶The Health and Retirement Study and the Panel Study of Income Dynamics collect data on both parents and adult children living out of the household. However, neither has sufficient sample size for this analysis.

⁷There is also a literature documenting how the mandate affected firms and workers. Premiums for plans covering dependents increased (Depew and Bailey 2015), and workers in firms with employer-based coverage saw a reduction in wages (Goda et al. 2016).

parents' labor supply decisions. In this case, job lock from dependent health insurance induced a large number of parents to delay retirement. The relatively large effect on parental retirement suggests that dependent coverage is as important, if not more so, as own coverage in terms of generating job lock. The desire to provide for adult children rivals the incentive to retire. My findings complement growing evidence that parents still financially support their children into adulthood by demonstrating that parents are also willing to delay retirement in order to provide for their adult children. Together, they highlight the importance of taking into account spillovers onto parents when considering policies targeted at young adults.

The rest of the paper is organized as follows: in Section 2, I describe the reform and lay out a simple conceptual framework. In Section 3, I discuss the data and identification strategy. I present my results in Section 4, and discuss their implications and conclude in Section 5.

2 Policy Change and Conceptual Framework

The dependent coverage mandate was one of the most popular and well-publicized components of the Affordable Care Act (Goldman 2013). This mandate required insurance plans with dependent coverage to expand coverage to children up to age 26. Before the ACA mandate, insurance plans only covered dependents up to age 18 or until they were out of college. Some individual states had their own dependent coverage mandates, but eligibility requirements varied widely by the dependent's marital status, student status, or own employer-sponsored insurance, and almost none required coverage up to age 26.⁸ In contrast to individual state policies, the ACA dependent coverage mandate was a national policy with no eligibility requirements besides age. Any young adult under 26 whose parent's health insurance covered dependents could now obtain coverage under her parent's plan. The policy was announced in March 2010 and insurers were required to comply by September 2010. In March 2010, the Internal Revenue Service also amended its rules to allow health benefits for dependents to be tax-exempt up until the dependents reached age 27 (Internal Revenue Service 2010). The dependent mandate had a significant impact on insurance rates for young adults. About 2 million young adults (7

⁸See Depew (2015), Table 1 for an overview of state policies.

percent of adults aged 19-26) added parental employer-sponsored insurance as a result of the mandate (Antwi et al. 2013). By 2016, 26 year olds were 1.19 times more likely to be uninsured than 25 year olds (Barnett and Berchick 2017).

Of course, for every child who gained insurance through this mandate, there was a parent who added them to their insurance plan. Therefore in families where parents were on employer-sponsored insurance plans, the mandate established a link between the adult children's health insurance coverage to their parent's job. The mandate's age cutoff at 26 presents us with a quasi-experimental setting to study the extent to which parents are willing to adjust their labor supply on behalf of their adult children. Given that most of the affected parents were in their 50s or 60s,⁹ a relevant labor supply outcome to study would be retirement.

Next, I lay out a conceptual framework based on Gruber and Madrian (2004) to illustrate how the dependent mandate could induce job lock for parents and cause them to delay retirement. Assume that workers with inelastic labor supply decide between two states: working with insurance, or not working without insurance. For simplicity, assume that health insurance is tied to working (i.e., there is no retirement insurance, Medicare, Medicaid, or non-employer private insurance). Let utility at age t be $U_t(w, H, L)$, where w is the compensation at the current job, H is a dummy variable for health insurance coverage, and L is inelastic leisure. Utility is increasing in all three arguments. Since health insurance is tied to working, define $H = \mathbb{1}\{L = 0\}$. w and L are related as follows:

$$\begin{cases} w > 0 & \iff L = 0 \\ w = 0 & \iff L = 1 \end{cases}$$

Each year, individuals simply compare working with health insurance, $U_t(w, 1, 0)$, to not working without health insurance, $U_t(0, 0, 1)$. If $U_t(w, 1, 0) \geq U_t(0, 0, 1)$, then they work this year.

Now, introduce a reform which changes the utility function as follows:

$$U'_t(w, H, L) = \begin{cases} U_t(w + B - CD, H, L) & \text{child} \leq 26 \\ U_t(w, H, L) & \text{child} > 26 \text{ or no child} \end{cases}$$

⁹The average age of parents in my sample (parents of 23-29 year olds in 2010) was 58 at the end of the panel.

where $B \geq 0$ represents the value of dependent health insurance to the parent and $CD \geq 0$ is the compensating differential by which an employer reduces the parent's compensation.¹⁰ If workers now value employer-sponsored health insurance more, firms can lower their wage until their utility is back to the pre-reform level and capture this rent.¹¹ If $w + B - CD > w \iff B - CD > 0$, then the reform increases the value of continuing to work for affected parents.

Parental labor supply is unaffected by the mandate if $B - CD = 0$. This could happen in two ways. First, if parents don't factor their adult children into their retirement decision, then they value the dependent coverage benefit at $B = 0$ and employers would set $CD = 0$. Alternatively, even if parents do factor their children in and value their dependent's coverage at $B > 0$, firms could still set $CD = B$ and exactly compensate for it. In both cases, the workers' labor supply will be unaffected and the reform will not induce job lock.

Job lock occurs when $B - CD > 0$, meaning workers value the dependent benefit and employers do not set compensating differentials to exactly offset how much the worker values it. Then, there could be workers for whom $U_t(w + B - CD, 1, 0) > U_t(0, 0, 1) > U_t(w, 1, 0)$. Put another way, absent the policy these individuals would have retired at age t ($U_t(0, 0, 1) > U_t(w, 1, 0)$), but now they continue to work in order to take advantage of the policy ($U_t(w + B - CD, 1, 0) > U_t(0, 0, 1)$). If we find causal evidence that affected parents delayed their retirements, this would imply that the additional value of the dependent insurance (net of the compensating differential) is high enough that they are willing to continue working when they otherwise wouldn't have. Thus if we observe job lock, we can conclude that $B > 0$ and parents did indeed factor this benefit for their adult children into the retirement decision.

¹⁰Goda et al. (2016) finds evidence that workers in firms offering dependent coverage saw annual wages decrease by \$1200.

¹¹Of course, if premiums paid by the employer went up as well (which indeed seems to be true according to Depew and Bailey (2015)), this would not be a rent per se as the cost to employers of providing insurance to workers increases. For the sake of simplicity in this model, I do not include the employer's decision of whether or not to offer health insurance. Additionally, I assume that $CD < B$, meaning employers do not impose compensating differentials higher than how much parents value dependent insurance.

3 Data and Identification Strategy

The data I use is the 2008 panel of the Survey of Income and Program Participation (SIPP). SIPP surveys a nationally representative sample of 42,000 American households for about 4 years, asking them to recall a variety of information from the past four months. The 2008 panel covers May 2008 to December 2013, which spans the pre- and post-policy period of the ACA. It collects data on demographics, employment status, assets and earnings, health and disability, government program participation, and job benefits. SIPP provides a rich monthly snapshot of work history during this period for every individual in a household.

Using the SIPP, I compare retirement and Social Security receipt for parents whose children are affected by the mandate to those who children are too old to be affected by it. To define my treatment and control groups, I will use the age of a respondent's youngest child. I define "treated" to mean that the respondent's youngest child is less than 26 in 2010, and "control" if the youngest child is older than 26. It is unclear whether children who were 26 in 2010 would be eligible or not, since insurance companies varied in when they began to comply with the mandate. So, following Antwi et al. (2013), I do not assign parents of 26 year olds to either treatment or control.

In order to have relatively comparable groups, I restrict my treatment group to parents of 23-25 year olds and control group to parents of 27-29 year olds. Note that a young adult who is 23-25 in 2010 will no longer be eligible for the mandate by the end of the panel in 2013. However, I still use their parents as a treatment group for the main analysis two reasons. First, the identifying assumption that the treatment and control groups are comparable may not hold when comparing parents of children with vastly different ages.¹² Holding all else equal, parents of much older children may have different retirement plans than parents of younger children, even absent this policy. Thus in order to keep the treatment and control groups as similar as possible, I use "partially treated" parents of 23-25 year olds as my treated group. Second, using "partially treated" parents gives us a lower bound on the actual effect of the mandate so the results will be, if anything, biased towards 0. In Section A I repeat the analysis using a "fully treated" group of parents and

¹²If the treated group was defined as parents of children who were unaffected in 2013, then the difference between child's ages in the treatment and control group would be at least 5 years and as high as 9 years. The "fully treated", (i.e., unaffected in 2013) group consists parents of children younger than 26 in 2013, and the control group's children (who were 27-29 in 2010) would be as old as 32 in 2013.

find similar results. In Table 1, I report summary statistics of individual characteristics by treatment status. In the main analysis, I then restrict the sample of parents to those aged 55-66 at a given point in time because of small sample sizes at older ages for the treatment group and at younger ages for the control group.

While the treatment and control groups are relatively similar in many dimensions, it is important to note that parents in the control group are older than those of the treatment group. This makes sense, since the control group comprises parents of older children. Age is an important factor in the retirement decision. Thus, the baseline retirement rate for the control group should be higher than that of the treatment group. Accordingly, we also see that control parents are less likely to be working in 2009 (Table 1). In order to address this, my empirical strategy will hold age fixed when comparing labor force outcomes between the treated and control groups.

It is also of note that the sample contains more women than men. Women comprise 64 percent and 63 percent of the treatment and control groups, respectively. This is because I do not directly observe every child's age for children no longer living in the household for both mothers and fathers. Instead, I use mothers' fertility history¹³ and consider the ages of the children a woman gave birth to. Fathers (or step-fathers) are then assigned to treatment or control depending on the fertility history of their wife. Thus, I can only add married men whose wives were also in the sample, which explains the gender imbalance. Note that both the treatment and control groups are equally unbalanced and have the same female share, and thus are comparable to each other.

I use responses only from individuals with valid interview status and nonmissing identifiers. I consider whether a respondent has a job in the past month, whether she is looking for a job, and her reason for not having a job. I consider a respondent to be retired if her employment status for a given month is "no job all month, no time on layoff and no time looking for work" and the main reason for not having a job in the reference period (last four months) is retirement. I also consider Social Security benefit receipt as another outcome.

The empirical strategy consists of comparing cross-sections of the outcome variable by age, and looking at the difference in the cross-sections for the two groups. For each

¹³SIPP only asks for the oldest and youngest children that a woman gave birth to. Thus I also have no sense of exactly how many eligible or ineligible children the respondent has, although I do know the total number of children. I also do not observe ages of adopted children or stepchildren, who would both count as dependents for insurance purposes. Not accounting for them will bias my result toward zero.

age, I first calculate the raw difference between the treated and control group’s cross-sections in the last period of the 2008 panel, about 3 years after policy enactment.¹⁴ The cross-section plots retirement rate as a function of age, and represents the percent of each group that is retired or receiving Social Security benefits at a given age at the end of the panel. This strategy allows me to hold age fixed while comparing the treated and control groups. It is important to make comparisons *within* age cohorts since the retirement decision is tightly linked to age, and the control group is on average older than the treated group. Then in a regression framework, I control for individual characteristics that could contribute to the raw difference between treatment and control in the cross-section. Importantly, this regression includes an age fixed effect in order to compare retirement rates within age cohorts.

I argue that the coefficient on the treated dummy captures the causal effect of the dependent mandate on the outcome variable. The identification assumption underlying the comparison between treated and control groups is that absent the mandate, there would be no difference in outcomes between treated and control parents *at a given age*. This would be true if having a child slightly older or younger than 26 in 2010 is as good as random. With this assumption, any observed difference reflects the causal effect of the mandate. I then conduct a series of placebo tests to support this identification assumption and argue for a causal interpretation of the results.

An alternative strategy might be to use a difference-in-differences identification strategy. However, using a difference-in-differences framework would require making a parallel trends assumption that the outcomes of the two groups would have evolved similarly in the post-policy period absent the mandate. In the context of retirement, where a large proportion of workers retire once they hit statutory ages like 63 and 66, it may not be reasonable to assume that the retirement rate of an older group would trend similarly to that of a younger group. This poses a problem when comparing parents of older children to parents of younger children, because on average the former is older than the latter. Indeed, if we consider the retirement rate for the two groups in 2008 in Figure 1 (left panel), we see that in the post-policy period the control group’s retirement rate increases faster than that of the treatment group. In a difference-in-differences framework, this would

¹⁴In the 2008 SIPP panel, individuals exit the sample at different points in time. I restrict the analysis to individuals whose last observation is at least 1 year after policy implementation (i.e., whose last observation is on or after September 2011). This is true for 75 percent of individuals. Of this group, 83 percent of individuals’ last observation is on or after June 2013.

suggest that the mandate caused the treated group to delay retirement. But we see the same pattern if we compare two similarly-defined groups in 2004 data in the absence of the mandate (Figure 1, right panel), which violates the parallel trends assumption. Since a difference-in-differences strategy cannot simultaneously control for age and time, it is difficult to disentangle whether the difference in retirement rates after the mandate is due to the different age compositions of the two groups or due to the mandate. But once I restrict to cross-sectional comparisons holding age fixed, then any difference in outcomes between the two groups cannot be due to differences in age composition between the two groups. Thus, the method of comparing cross-sections of retirement profiles is preferable to a difference-in-differences framework.

Finally, I quantify the delay in retirement induced by the mandate in years, rather than probability of being retired. One challenge to estimating this directly in a regression framework is that the 2008 SIPP panel ends in 2013, before many parents in the sample have retired. Retirement age is censored for most parents in my sample and thus I cannot use observed retirement age as an outcome variable directly in the regression without modelling censorship or duration. Instead, I opt for a simpler measure – I consider how much the dependent mandate “shifted” the treated group’s retirement profile to the right, relative to the control group’s profile. In practice, I measure the horizontal distance between each group’s retirement cross-sections at different retirement rates. While the previous set of results looked at retirement rate as a function of age, with this measure I consider age as a function of retirement rate. This non-parametric method allows me to estimate the difference in retirement age between the two groups without having to introduce a more complicated model of duration or censorship. If we assume rank preservation, meaning that an individual would be in the same retirement age quantile regardless of whether she is in the treated or control group, then the horizontal distance between the two retirement profiles represents the retirement delay induced by the dependent mandate measured in years.

4 Results

4.1 Main Results

Figure 2 plots the average retirement rate in the post-policy period by age and treatment group. I use each individual's outcome in their last observation in the 2008 panel, which occurs in 2013 for the majority of individuals in my sample. I subset to individuals who exit the panel at least one year after policy implementation (i.e., after September 2011). Age is defined as the person's age in the last observation, rounded to the closest integer. Since I compare individuals who exit the panel at the same age, the gap in retirement rates between the two groups represents the difference in the percent of individuals who are retired by a given age.

I next quantify the raw difference in retirement rates between the two groups by taking repeated bootstrap samples to obtain confidence intervals; Figure 3 plots the results for by age. There are no differences in retirement rates for parents in their late 50s, but a gap emerges from age 60 on. The point estimates of the raw differences at each age are reported in Table 2. After age 60, the individual age cohort estimates of the difference in retirement rates are all negative, and as large as 12 percentage points in magnitude. For ease of interpretation and to improve power, I aggregate the estimates from separate age cohorts into groups (i.e., 55-60, 61-66, and 55-66), which are also reported in Table 2. The group average difference is calculated by estimating separate differences for each age cohort and taking the mean of the estimates, rather than pooling individuals in each group together and calculating the difference. Taking the average of the estimates rather than pooling ensures that the estimates are not a result of differing age compositions between the treatment and control groups. On average for all individuals between 55 and 66, the retirement rate is 5.4 percentage points, or 25.9 percent, lower in the treated group compared to the control group. This is driven by individuals in the 61-66 age group, where the retirement rate is 9.3 percentage points (28.0 percent) lower for the treated group. The average difference is statistically significant for the overall group and for the 61-66 age group, as the 95% bootstrapped confidence intervals for the difference fall below 0.

Next, I run a regression of the treatment dummy on retirement, controlling for individual characteristics and age fixed effects. The results are reported in Table 3. Even after

taking into account demographic characteristics, education, and pre-reform labor supply, the gap in retirement between treated and control remains negative and statistically significant. On average between the ages 55 to 66, the treated group is 3.8 percentage points (18.2 percent) less likely to be retired. Again, this gap is driven by the large effects for treated individuals aged 61-66, who are 8.0 percentage points (25 percent) less likely to be retired compared to control parents in the same age group.

I can extrapolate the regression results to the overall US population and calculate how many workers delayed retirement in response to the dependent mandate. Using the age profiles of all affected parents in my sample,¹⁵ I calculate that there were about 7.6 million parents of 19-25 year olds who were between 55 and 66 in 2013. Combined with the estimates from the main regression results, this implies that about 291,000 parents delayed retirement in order to take advantage of the mandate. In comparison, Antwi et al. (2013) estimated that about 2 million young adults adults gained parental employer-sponsored insurance after the mandate was implemented.

Next, I consider a regression specification using one-year age dummies by child's age, rather than grouping them together to form treatment and control groups. Figure 4 plots the coefficients of a regression of a dummy for child's age on retirement, with parents of 27 year olds as the omitted group. The left panel plots the coefficients for parents aged 55-60, and the right panel plots the coefficients for parents aged 61-66. In both panels, the coefficients are negative for those with children 26 and under, but insignificant and close to 0 for parents with children over 27. The jump the coefficients at age 27 is in line with parents' ineligibility for the mandate after age 26.

Overall, the results on retirement demonstrate that among parents over 60, those eligible for the mandate were less likely to be retired than ineligible parents post-policy. Because we hold age fixed, the difference in retirement rates is not due to the differing age composition between the treated and control groups. Since other covariates are relatively balanced across the two groups (Table 1), adding them as controls does not change the findings much from the comparison of raw differences. Thus, the difference in retirement rates between the two groups is not driven by parental age composition or other parental covariates.

¹⁵Within SIPP, I calculate the percentage of each age cohort in 2010 that has a child aged 19-25. I then combine this with population count by age in the 2010 Census, and extrapolate to calculate the total number of individuals who are parents of a 19-25 year old and were between 55 and 66 in 2013.

The two groups do, however, differ mechanically in one important way – the children of treated parents are younger than the children of control parents. If parents of younger children are less likely to retire in their early 60s than parents of older children, then we would see this pattern in retirement rates even without the mandate. Since treatment and control are defined by children’s ages, there is no way to disentangle this by simply controlling for it.

In order to address the issue of differing children’s ages, I conduct a series of placebo tests to check whether differences in children’s ages could be driving the difference in retirement rates between the two groups. The first placebo test considers treated and control parents in the pre-period – that is, it takes a cross-section of retirement rates by age in *September 2010*, right before the policy was implemented. If control parents’ higher retirement rate is driven by the fact that their children are older, then their retirement rate should be higher before implementation of the mandate as well. Figure 5 plots the differences by age and Table 4 reports the regression results. For all groups, the difference in retirement rates between treated and control individuals is statistically insignificant. Thus, the observed difference in retirement rates between the treatment and control group emerges only after the dependent mandate is implemented.

The second placebo test considers a group of parents that should not be affected – parents whose children are 31-33 in 2010. This placebo group of parents has children with a similar age difference to the control group as the treated group does, albeit in the opposite direction. If the differences in the results were driven mostly by the fact that the control group has older children than the the treatment group, then we would expect to see differences in this comparison, with the placebo group having higher retirement rates than the control group. Figure 6 plots the differences by age and Table 5 reports the regression results. For all groups, the coefficient is insignificant, albeit negative. However, note that the placebo group has children *older* than those of the control group. If the main results are driven by parents with older children being more likely to be retired, then the placebo parents would be more likely to be retired and thus we would expect the coefficient to be positive. Additionally, in the plot of raw differences in Figure 6, we see that the negative average difference is driven by a potential outlier at age 65, rather than negative differences at every age over 60, as seen with the main results in Figure 3.

The third placebo test considers the effect of a placebo policy in 2004 using a previous

SIPP panel. I define treatment and control groups using parents in the 2004 SIPP panel and compare their retirement rates at the end of the panel in 2007. Specifically, treatment is defined as having a child aged 23-25 in 2004 and control is defined as having a child aged 27-29 in 2004. This definition mirrors that of the 2008 panel because the “policy” occurs about 3 years before the last observation; in the 2008 panel, the policy occurs in 2010 and the last observation usually occurs in 2013. Similar to the first placebo test in the pre-period, since there was no mandate in effect in 2007, any difference between the two groups would imply that the main results are driven by something other than the dependent mandate. Figure 7 plots the differences by age and Table 6 reports the regression results. The coefficients are all statistically insignificant.

All three placebo tests are insignificant, indicating that the difference in retirement rates between treatment and control in Table 3 is not driven by the differing age compositions of each group’s children. Additionally, controlling for individual characteristics does not change the estimates much (Table 3), meaning they are not driving the difference either. The difference only emerges in the post-policy period, and only for groups that were differentially affected by the reform (i.e., 23-25 vs. 27-29 opposed to 31-33 vs. 27-29). Taken together, the main results and placebo tests suggest that the gap in retirement between treated and control parents is caused by the dependent mandate, indicating that treated parents eligible for dependent health insurance delayed retirement to take advantage of it.

Since the retirement decision is closely linked to eligibility for Social Security and many of the ages with a gap in retirement rates are Social Security-eligible ages, I next turn to receipt of Social Security benefits as an outcome. Figure 7 plots the difference by age in the last observation in the 2008 panel. A gap in Social Security receipt rates emerges at age 63, which is the early Social Security eligibility age, but it is imprecisely estimated. After aggregating these differences by age group in Table 7, the differences are statistically insignificant. Similarly in the regression results in Table 8, the coefficients are statistically insignificant. Thus, I cannot reject the null hypothesis that the mandate did not affect Social Security receipt rates. One possible explanation for why I find an effect on retirement but not Social Security receipt is that the gap in retirement rates emerges as early as age 60, before parents would be eligible for Social Security retirement benefits. It could be that absent the mandate, parents aged 60-62 would have retired

but would not have been able to receive Social Security benefits – thus, the mandate caused them to delay retirement, but had no effect on when they started receiving Social Security.

In Appendix Section A, I consider a series of alternate specifications to confirm the robustness of the results. The results are robust to defining cohorts by birth year rather than age at last observation in panel, to defining treatment as being “fully treated” (i.e., parents of children who are never eligible for the mandate in the panel because they are younger than 26 in 2013), and to changing the bandwidth of children’s ages for defining treatment and control groups.

4.2 Quantifying Retirement Delay

The main results demonstrate that the ACA dependent mandate decreased the likelihood of retirement for eligible parents, especially for those in their 60s. Since almost all individuals will eventually retire, this decreased probability eventually translates into a delay in retirement age. I next calculate the length of retirement delay in years implied by my results. One challenge is that retirement age is censored for many individuals because the SIPP panel ends in 2013. So, I cannot use retirement age directly as an outcome variable in the regression framework used in the main results.

Instead, in Figure 9 I calculate how much “longer” it took the treated group to reach a given retirement rate compared to the control group. Rather than looking at how the mandate affected retirement at a given age, I consider how it affected the average age at a given retirement rate. Doing this gives the retirement delay induced by the mandate as measured in years, without having to introduce a duration or censorship model of retirement age.

I construct a cross-section of retirement profiles by age after September 2010, utilizing additional observations of an individual’s monthly work history between policy implementation and the individual’s last observation in the panel. Including this additional information allows for a more granular measure of retirement rate – by using the full set of post-policy observations, I measure retirement rate at every age in months, rather than rounding to age in years as in Figure 2. This additional information about each group’s retirement profiles is otherwise difficult to use in a regression approach without a more complicated duration or censorship model. The bootstrap method for estimating

confidence intervals is clustered at the individual level to ensure that inference is still at the individual level. Each point on this retirement profile at a given age represents the percent of the treated or control group that was retired by that age in the post-policy period. Thus, the horizontal distance between the two profiles at a given quantile represents how much longer it took the treated group to reach that retirement rate relative to the control group. For example, 10 percent of the control group had retired by age 58.73, while it took until age 60.25 for the treated group to reach 10 percent retired. Thus, the treated group required 1.52 more years to achieve a 10 percent retirement rate relative to the control group.

In order to estimate the horizontal distance between the two retirement profiles, I use a LOESS (locally estimated scatterplot smoothing) regression with smoothing parameter of 0.15 to fit a smooth curve to each group's retirement profile. I then calculate the horizontal distance between the smoothed retirement profiles at each quantile on the y-axis. In order to estimate bootstrapped confidence intervals, I resample the data at the individual level and recalculate the LOESS regression fitted line and horizontal distances. The left panel of Figure 9 plots the original cross-section and the average of fitted lines from 1000 resamples. The right panel of Figure 9 plots the horizontal distance between the 1st and 50th quantile and corresponding 95% bootstrapped confidence intervals.

The difference is positive for almost every quantile of retirement rate and statistically significant at the 95th percent confidence level between the 9th and 18th quantiles. Between the 1st and 50th quantiles, the average difference between treated and control retirement ages was 0.74 years. The largest statistically significant difference was 1.7, at the 9th quantile. If we assume rank preservation, meaning that an individual would be in the same retirement age *quantile* regardless of whether she is in the treated or the control group, then we can interpret this as a treatment effect at each quantile. This would imply that the mandate caused treated parents to delay their retirement by 0.74 years. If we do not assume rank preservation, this means that on average it took 0.74 years longer for the treated group to reach a given retirement rate between 1 percent and 50 percent, relative to the control group.

5 Discussion

Compared to previous literature, the finding that treated parents were 3.8 percentage points less likely to be retired and delayed retirement by 0.74 years in response to the dependent mandate is relatively large. Madrian et al. (1994) found that retirement health insurance availability led workers to retire 0.4-1.2 years earlier. French and Jones (2011) estimated that hypothetically raising the Medicare age to 67 would cause only 0.07 years of retirement delay. Brown (2013) found that in response to a 10 percent increase in the return to work, individuals delayed retirement by 0.17 years.

There could be a few factors that contribute to the size of this effect. First, the policy change I consider occurred in the wake of the Great Recession. More parents may have been on the margin of delaying retirement in this time period than normal. Goda et al. (2011) found that after the recession, a 10 percent decrease in the level of the S&P index increased the probability of working after age 62 by 1.21 percentage points. They note that this is quite large relative to estimates from other periods, suggesting that the decision to delay retirement was more responsive to incentives in this period than in other years. Adult children were also worse off than usual in terms of insurance coverage after the recession. Young adults disproportionately lost insurance in the recession – 2.5 million (3.1 percent) adults aged 19-34 became uninsured from 2007 to 2009, the largest increase in uninsured rate across all nonelderly adult age groups (Holahan 2011). This means that more adult children may have relied on their parents for insurance coverage in this period. The coincidence of more parents on the margin of delaying retirement and more young adults losing health insurance in the Great Recession could explain the relatively large response to the ACA dependent mandate.

Second, the dependent mandate was well-publicized and had a straightforward eligibility cutoff of 26. There is evidence of large “statutory age effects” on retirement which are independent of financial incentives, meaning that simply defining a statutory retirement age can affect workers’ retirement behavior (Seibold 2017). Over 70 percent of the public was aware of the dependent mandate within a month of enactment (Kaiser Family Foundation 2010), and it was one of the most popular components of the ACA. Thus, the cutoff age of 26 could have served as a salient “statutory age” around which parents planned their retirement.

Third, most papers in the retirement decision literature focus on policies about in-

centives for workers themselves, who in this case would be the parents. But the policy studied here affected the *children* of these workers. In the context of the large literature on how children affect parental labor supply (e.g., Angrist and Evans 1998; Kleven et al. 2018; Bertrand et al. 2010), my findings fall in line as they expand the age range of children in question and highlight the important role that *adult* children also appear to play in parental labor supply.

These findings also raise additional questions. I show empirical evidence that parents take their adult children into account in their retirement decisions. These empirical findings point to the need for a model of retirement which takes into account not only an individual's own incentives and spousal incentives but also incentives formed by children. Looking beyond the individual workers affected by mandate, we may also wonder about its impact on firms. Throughout this paper, I implicitly assumed that firms could not respond to the mandate by reducing their compensation or encouraging them to retire in other ways. If this was true, it would attenuate my job lock finding. However, it has been shown that the mandate increased premiums for insurance plans covering dependents (Depew and Bailey 2015) and that workers in firms offering dependent coverage saw annual wages decrease by \$1200 (Goda et al. 2016), suggesting the compensating differential in response to this policy was not zero. This would imply that my results are biased towards zero relative to a scenario where firms did not respond by changing wages. Given this paper's finding that firms also had to accommodate workers delaying retirement, it would be interesting to explore how this change in worker labor supply affected firms.

In summary, I use the ACA dependent mandate to estimate the effect of dependent insurance on parental retirement rates. I find that the availability of dependent coverage significantly decreased the likelihood of retirement for eligible parents. Dependent health insurance can generate job lock for parents, even when the dependents themselves are adults. On average, dependent insurance coverage reduced the retirement rate among 55-66 year old parents eligible for the mandate by 3.8 percentage points after policy enactment, which is 18.2 percent of the retirement rate of the control group. Assuming rank preservation of retirement timing, this implies that in response to the mandate, eligible parents delayed retirement by 0.74 years on average to take advantage of the dependent mandate. Overall, about 290,000 parents delayed retirement to take advantage

of the policy. Altogether, these findings indicate that adult children still play a major role in their parents' labor supply decisions. Even when their children are adults, parents continue to make sacrifices for the benefit of their kids, even at the expense of giving up some of their "golden years" in retirement. These findings demonstrate the potentially large spillovers of policies aimed at young adults on their parents, and highlight the importance of taking these spillovers into account when making policy decisions.

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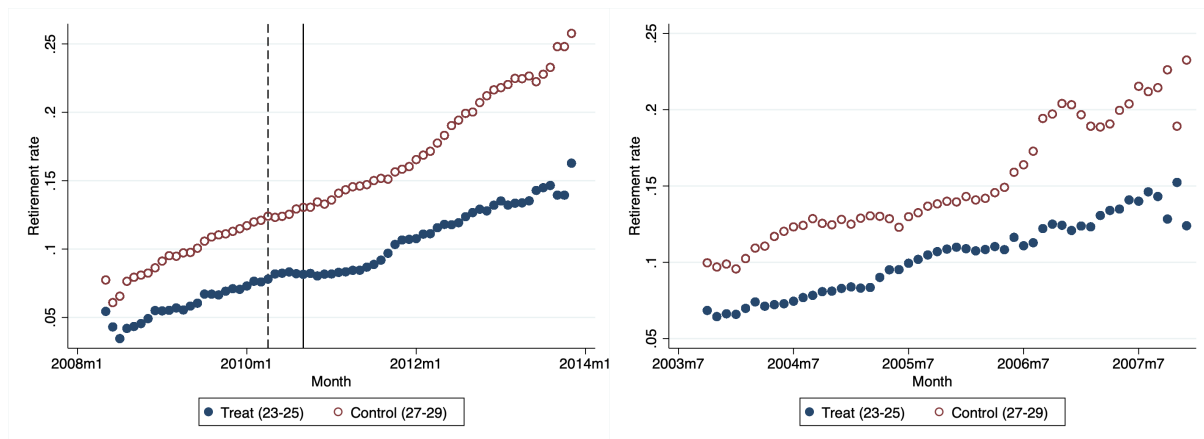
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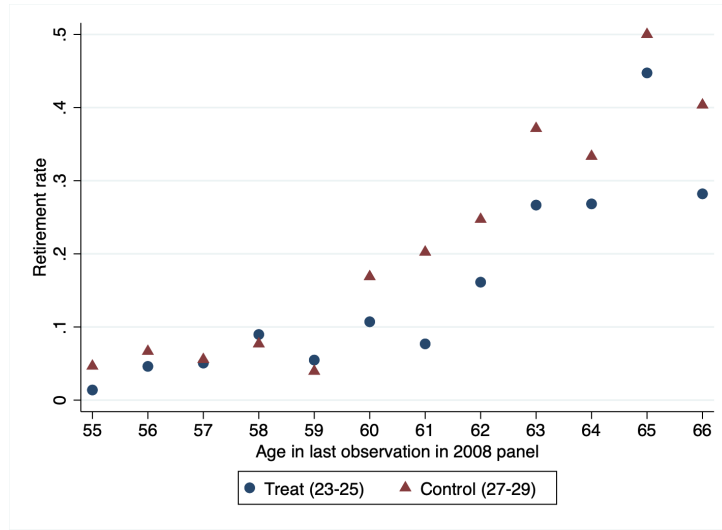
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6 Figures



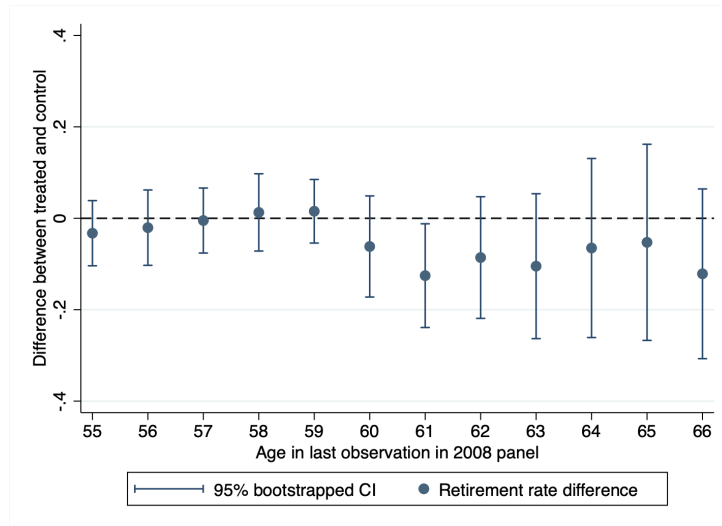
Dotted line represents policy announcement and solid line represents policy implementation of ACA dependent mandate. Treatment in 2008 SIPP (left) is defined as having a child aged 23-25 in 2010, and control is having a child aged 27-29. Treatment in the 2004 SIPP (right) is defined as having a child aged 23-25 in 2004, and control is having a child aged 27-29.

Figure 1: Percent retired by treatment and control, 2008 SIPP (left) and 2004 SIPP (right).



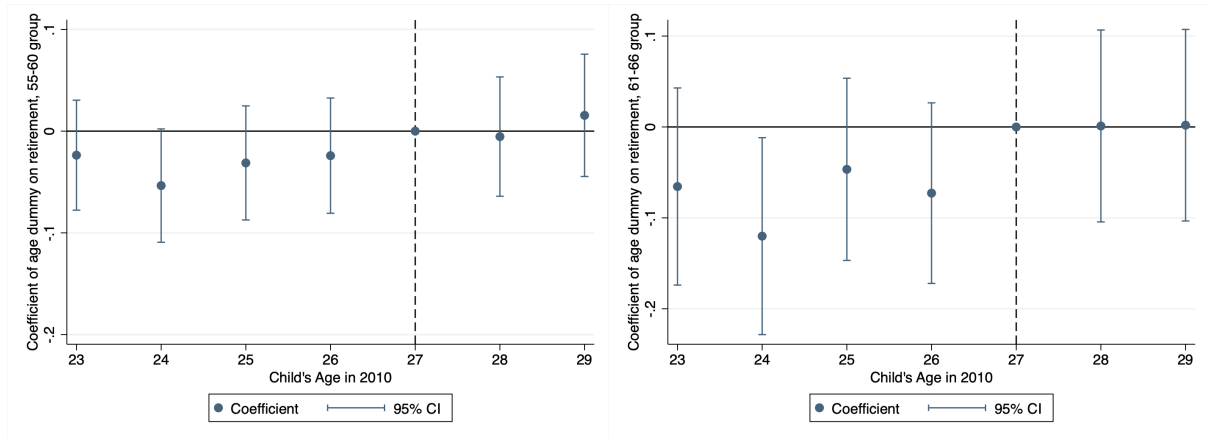
Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation.

Figure 2: Percent of individuals retired by age and treatemtn group in their last obser-
vation in the 2008 panel.



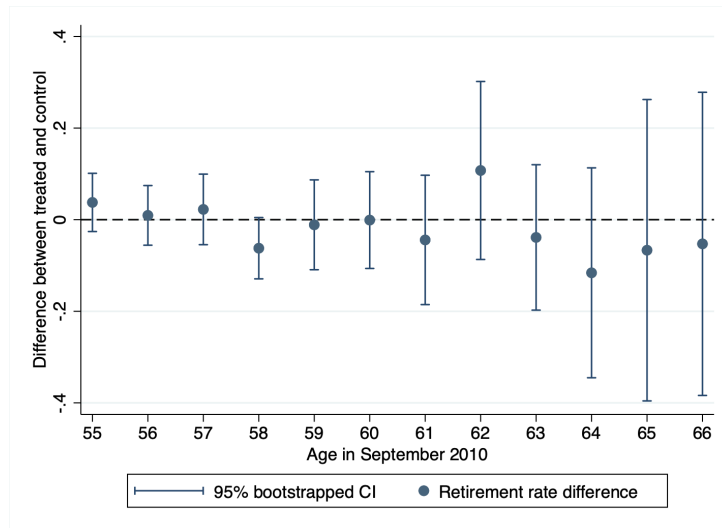
Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 3: Difference in average retirement rate between treated and control groups by
age in their last observation in the 2008 panel.



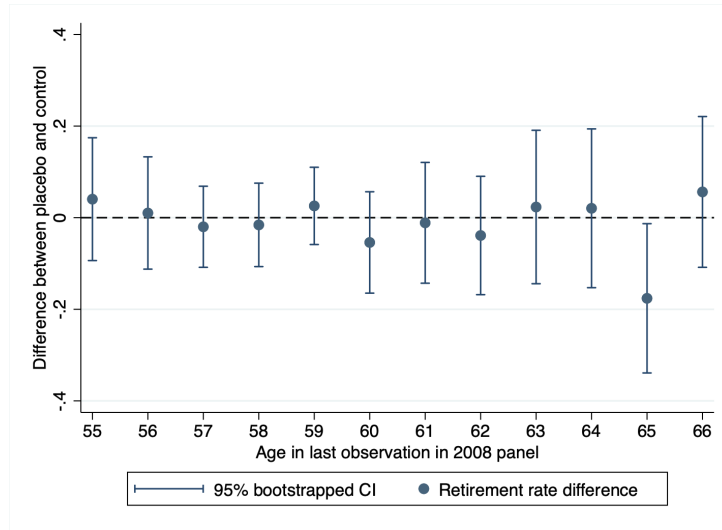
This figure plots the coefficients of regression of a dummy for child's age in 2010 on retirement in the last observation in 2008 SIPP panel, for parents with children between the ages of 23-29 in 2010. The omitted group comprises parents with children age 27 in 2010. Regression includes age fixed effect, demographic, educational, and pre-reform employment controls. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 4: Coefficient of dummy for child's age in 2010 on retirement in last observation in 2008 panel, for parents aged 55-60 in last observation in 2008 panel (left) and parents aged 61-66 in last observation in 2008 panel (right)



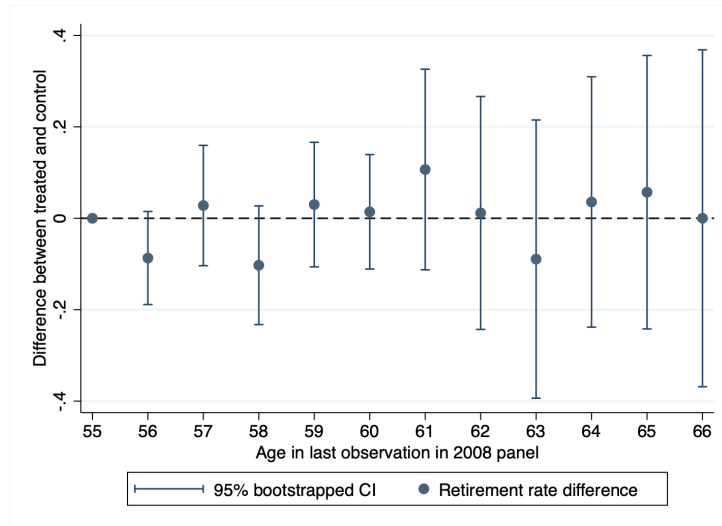
Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 5: Placebo test 1: Difference in average retirement rate between treated and control groups by age in September 2010 (before policy implementation).



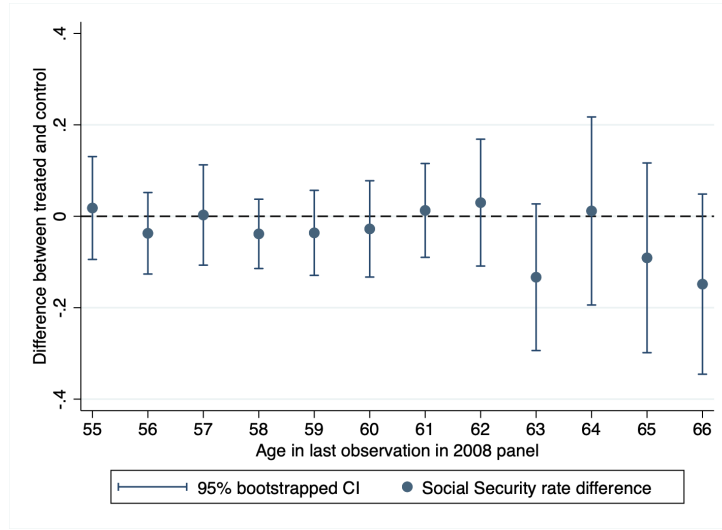
Difference calculated as placebo average minus control average. Placebo is defined as having a child aged 31-33 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 6: Placebo test 2: Difference in average retirement rate between placebo (child aged 31-33) and control (child aged 27-29) groups by age in their last observation in 2008 panel.



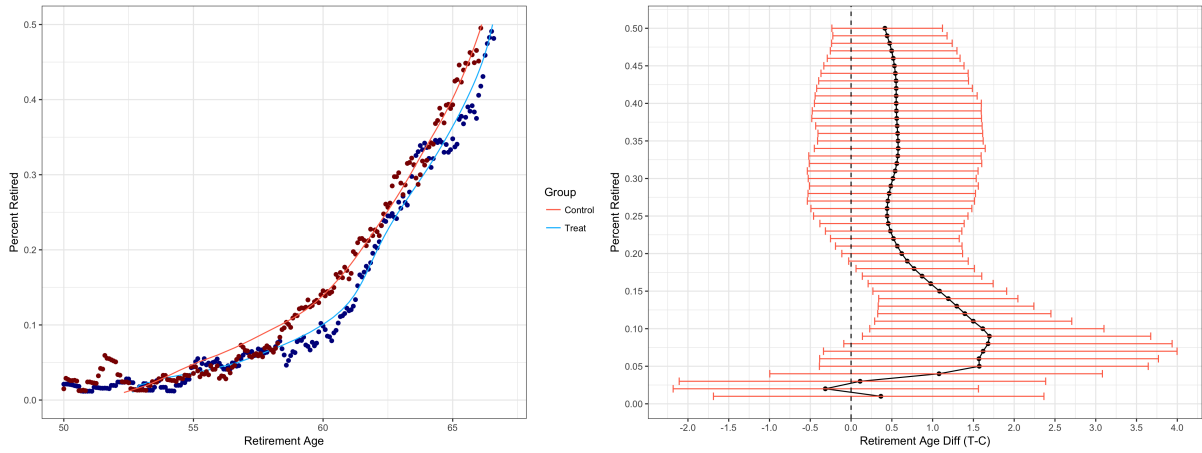
Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2004, and control is defined as having a child aged 27-29 in 2004. Sample restricted to individuals who exit the panel on or after May 2006. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 7: Placebo test 3: Difference in average retirement rate between treated and control groups by age by last observation in 2004 panel.



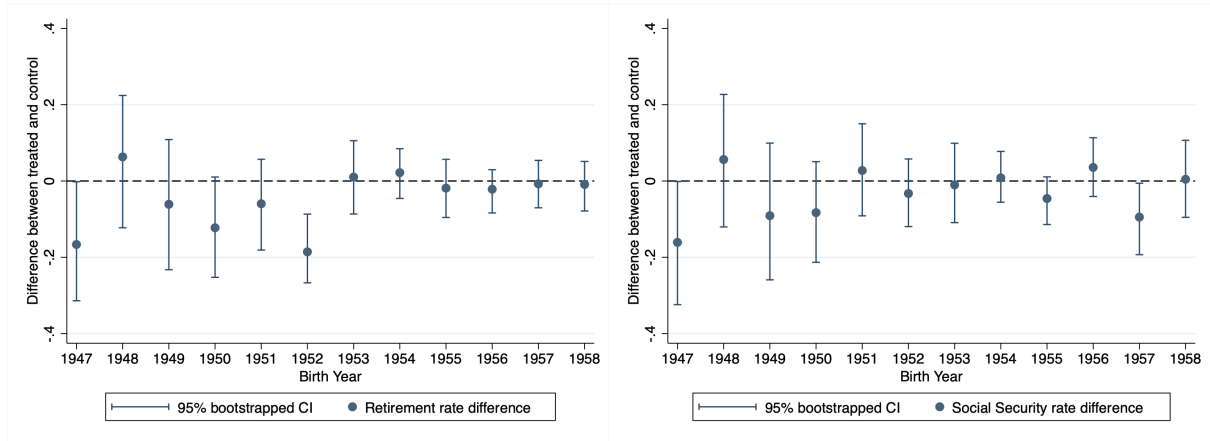
Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 8: Difference in Social Security receipt rate between treated and control groups by age in their last observation in 2008 panel.



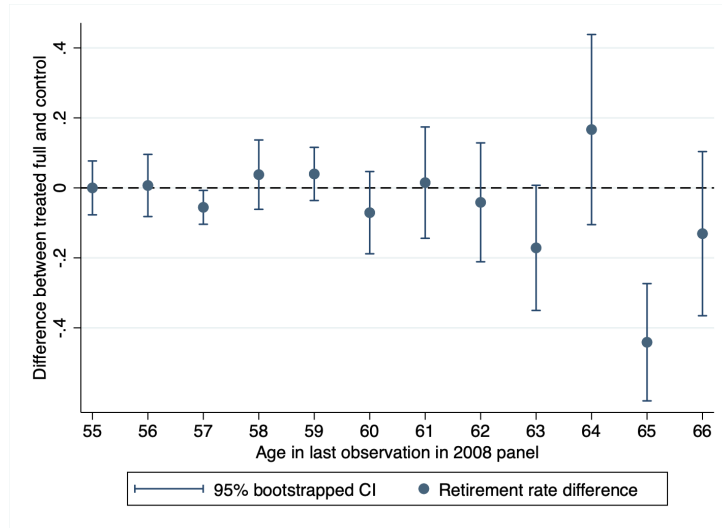
Cross-section is constructed using retirement rates for treated and control group individuals in the post-policy implementation period after September 2010. A LOESS regression (span = 0.15) is used to fit a bootstrapped smooth line to the retirement profile for the treated and control groups. The horizontal distance between the fitted lines (treatment minus control) is calculated between retirement rates of 1 percent and 50 percent. Confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$)

Figure 9: Percent retired by age in post-policy period (raw data) and non-parametric LOESS regression fitted lines (average of $N=1000$ resamples) (left); Horizontal retirement age difference by quantile and bootstrapped 95% confidence intervals ($N=1000$) (right)



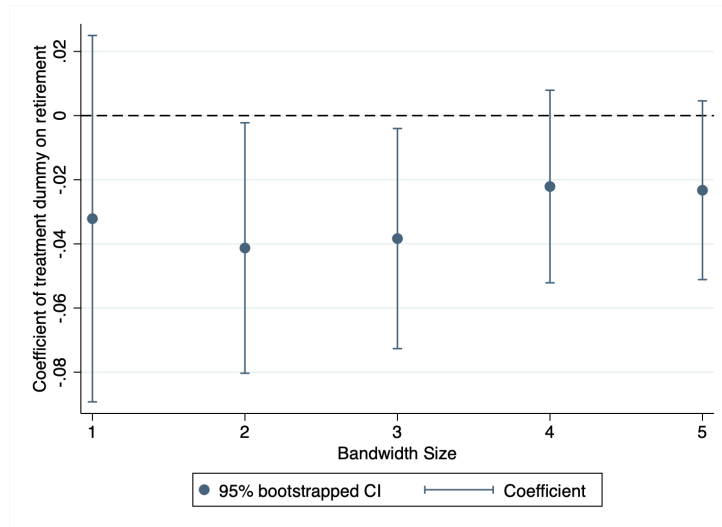
Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011, and are 55-66 in their last observation. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 10: Difference in retirement rate between treated and control groups (left) and in Social Security receipt rate between treated and control groups (right) by birth year in last observation of 2008 panel.



Difference calculated as treated average minus control average. Treated (full) is defined as having a child aged 20-22 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011 and were born between 1947 and 1958. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 11: Difference in retirement rate between fully treated (child aged 20-22 in 2010) and control groups by age in the last observation in the 2008 panel.



This figure plots the coefficients of a regression of a treatment dummy, where treatment is defined with bandwidths for child's age of varying size, on retirement. The treated group comprises of parents whose children under 26 in 2010, and the control group comprises of parents whose children are over 26 in 2010. Regression includes age fixed effect, demographic, educational, and pre-reform employment controls. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in their last observation in the panel. 95% confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Figure 12: Coefficient of treatment definitions with varying bandwidths on retirement in the last observation in the 2008 panel.

7 Tables

Characteristics	Type	Treat		Control	
		<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>
White	percent	0.84	0.36	0.83	0.38
Female	percent	0.64	0.48	0.63	0.48
Hispanic	percent	0.09	0.29	0.07	0.25
Married	percent	0.78	0.41	0.78	0.41
High School or greater	percent	0.89	0.32	0.9	0.3
College or greater	percent	0.40	0.50	0.39	0.49
Private HI in 2009	percent	0.80	0.40	0.81	0.395
Employed in 2009	percent	0.72	0.44	0.70	0.46
Age in 2009	mean	53.75	5.95	56.31	5.21
Number of children	mean	2.36	1.18	2.30	1.08
Monthly income in 2009 (>0)	mean	2970	3938	2556	3422
Observations	#	72747		71580	
Individuals	#	1374		1304	
Months Observed	#	52.95		54.89	

Table 1: Summary statistics by treatment and control

Age	N	Diff. in retirement rate (T-C)	95% bootstrap CI	Control retirement rate	$\frac{\text{Difference}}{\text{Control avg}}$
55	115	-0.033	(-0.104, 0.039)	0.047	-0.701
56	125	-0.021	(-0.103, 0.062)	0.067	-0.308
57	151	-0.005	(-0.076, 0.066)	0.056	-0.089
58	156	0.013	(-0.072, 0.097)	0.077	0.167
59	149	0.015	(-0.054, 0.085)	0.039	0.388
60	133	-0.062	(-0.172, 0.049)	0.169	-0.365
61	136	-0.125	(-0.239, -0.012)	0.202	-0.620
62	151	-0.086	(-0.219, 0.047)	0.247	-0.348
63	130	-0.105	(-0.263, 0.054)	0.371	-0.282
64	104	-0.065	(-0.261, 0.131)	0.333	-0.195
65	104	-0.053	(-0.267, 0.162)	0.500	-0.105
66	96	-0.121	(-0.307, 0.064)	0.404	-0.301
55-60	829	-0.015	(-0.049, 0.019)	0.079	-0.194
61-66	721	-0.093	(-0.164, -0.021)	0.331	-0.280
55-66	1550	-0.054	(-0.094, -0.014)	0.208	-0.259

Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in their last observation. The average difference for age ranges (i.e., 55-60) is calculated by estimating separate differences for each individual age cohort and taking the mean of the estimates. Confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 2: Difference in average retirement rates in last observation in 2008 panel between treatment and control group by parent's age

Dep. variable: retired in last observation in 2008 panel			
	(1)	(2)	(3)
	<i>Parent's age in last observation in 2008 panel</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Treat (23-25 in 2010)	-0.0383** (0.018)	-0.00603 (0.017)	-0.0797** (0.032)
Monthly earnings (2009)	-0.00000928*** (0.000)	-0.00000298 (0.000)	-0.0000113*** (0.000)
Paid job all of 2009	-0.188*** (0.026)	-0.117*** (0.028)	-0.280*** (0.043)
Female	-0.00102 (0.021)	0.00532 (0.022)	0.00475 (0.036)
White (race)	0.0180 (0.022)	0.00248 (0.020)	0.0400 (0.043)
Hispanic (ethn.)	-0.0428 (0.035)	-0.0499* (0.027)	-0.0159 (0.075)
Married	0.141*** (0.029)	0.119*** (0.028)	0.167*** (0.055)
HS or higher	0.0101 (0.030)	0.0235 (0.027)	-0.0230 (0.069)
College or higher	0.00711 (0.020)	-0.0127 (0.021)	0.0314 (0.034)
Priv. HI all of 2009	0.0522** (0.023)	0.0186 (0.022)	0.0843* (0.047)
Num. children	-0.0168** (0.008)	-0.0129* (0.007)	-0.0198 (0.015)
Age FE	X	X	X
Observations	1550	829	721
Control avg.	0.208	0.079	0.331

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in the last observation in the panel. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 3: Effect of treatment on retirement rate in last observation in 2008 panel, with age fixed effect

Dep. variable: retired in September 2010			
	(1)	(2)	(3)
	<i>Parent's age in September 2010</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Treat (23-25 in 2010)	-0.000244 (0.016)	-0.00350 (0.016)	0.00542 (0.035)
Monthly earnings (2009)	-0.00000911*** (0.000)	-0.00000561*** (0.000)	-0.0000138*** (0.000)
Paid job all of 2009	-0.310*** (0.026)	-0.206*** (0.030)	-0.482*** (0.044)
Female	-0.0216 (0.019)	-0.0169 (0.019)	-0.0140 (0.038)
White (race)	-0.0271 (0.023)	0.00126 (0.019)	-0.103* (0.062)
Hispanic (ethn.)	-0.0398 (0.036)	-0.0409 (0.025)	-0.0248 (0.075)
Married	0.201*** (0.031)	0.165*** (0.032)	0.233*** (0.062)
HS or higher	0.0527 (0.032)	0.0786*** (0.022)	0.00780 (0.071)
College or higher	0.00950 (0.018)	0.00115 (0.017)	0.0428 (0.037)
Priv. HI all of 2009	0.00618 (0.025)	0.00442 (0.025)	-0.00573 (0.055)
Num. children	-0.00803 (0.008)	-0.0110 (0.007)	-0.00153 (0.014)
Age FE	X	X	X
Observations	1372	896	476
Control avg.	0.152	0.0641	0.301

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals aged 55-66 in September 2010. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 4: Placebo test 1: Effect of treatment on retirement rate in September 2010, with age fixed effect

Dep. variable: retired in last observation in 2008 panel			
	(1)	(2)	(3)
	<i>Parent's age in last observation in 2008 panel</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Placebo (31-33 in 2010)	-0.0297 (0.020)	-0.0216 (0.023)	-0.0438 (0.031)
Monthly earnings (2009)	-0.00000963*** (0.000)	-0.000000764 (0.000)	-0.0000158*** (0.000)
Paid job all of 2009	-0.237*** (0.027)	-0.139*** (0.033)	-0.306*** (0.042)
Female	0.00411 (0.024)	0.00159 (0.027)	0.0114 (0.033)
White (race)	0.000792 (0.027)	-0.00627 (0.027)	-0.000590 (0.040)
Hispanic (ethn.)	-0.0316 (0.038)	-0.0548* (0.030)	0.0107 (0.078)
Married	0.190*** (0.031)	0.148*** (0.031)	0.216*** (0.053)
HS or higher	0.0610* (0.032)	0.0675** (0.028)	0.0548 (0.067)
College or higher	0.0177 (0.023)	-0.0261 (0.025)	0.0473 (0.033)
Priv. HI all of 2009	0.0499* (0.027)	-0.0148 (0.026)	0.0972** (0.047)
Num. children	-0.0121 (0.010)	-0.0238** (0.010)	-0.00113 (0.016)
Age FE	X	X	X
Observations	1507	639	868
Control avg.	0.208	0.079	0.331

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Placebo is defined as having a child aged 31-33 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in the last observation in the panel. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 5: Placebo test 2: Effect of placebo (child aged 31-33 in 2010) relative to control (child aged 27-29 in 2010) on retirement rate in last observation in 2008 panel, with age fixed effect

Dep. variable: retired in last observation in 2004 panel			
	(1)	(2)	(3)
	<i>Parent's age in last observation in 2004 panel</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Placebo (23-25 in 2004)	0.00950 (0.026)	-0.0123 (0.026)	0.0295 (0.055)
Monthly earnings (2004)	-0.00000540** (0.000)	-0.00000212 (0.000)	-0.00000683 (0.000)
Paid job all of 2004	-0.265*** (0.039)	-0.128*** (0.046)	-0.389*** (0.065)
Female	0.237* (0.131)	0.0248 (0.187)	0.427** (0.187)
White (race)	-0.00464 (0.037)	0.0114 (0.033)	-0.0537 (0.079)
Hispanic (ethn.)	0.0343 (0.067)	0.00983 (0.057)	0.102 (0.172)
Married	0.245* (0.128)	0.0256 (0.187)	0.450** (0.184)
HS or higher	-0.0112 (0.042)	0.0584* (0.033)	-0.0934 (0.094)
College or higher	-0.00266 (0.029)	-0.0191 (0.029)	0.00922 (0.058)
Priv. HI all of 2004	0.135*** (0.035)	0.0379 (0.035)	0.212*** (0.070)
Age FE	X	X	X
Observations	629	368	261
Control avg	0.188	0.0779	0.315

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Treatment is defined as having a child aged 23-25 in 2004, and control is defined as having a child aged 27-29 in 2004. Paid job all of 2004 is a dummy variable equal to 1 if the individual held a paid job every month in 2004. Sample restricted to individuals who exit the panel on or after May 2006 and aged are 55-66 in the last observation in the 2004 panel. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 6: Placebo test 3: Effect of treatment (child aged 23-25 in 2004) on retirement rate in last observation in 2004 panel, with age fixed effect

Age	N	Diff. in Social Security rate (T-C)	95% bootstrap CI	Control Soc. Sec. rate	$\frac{\text{Difference}}{\text{Control avg}}$
55	115	0.018	(-0.094, 0.131)	0.093	0.194
56	125	-0.037	(-0.126, 0.052)	0.083	-0.446
57	151	0.003	(-0.107, 0.113)	0.111	0.025
58	156	-0.038	(-0.114, 0.037)	0.077	-0.500
59	149	-0.036	(-0.129, 0.057)	0.118	-0.306
60	133	-0.028	(-0.133, 0.078)	0.117	-0.236
61	136	0.013	(-0.090, 0.115)	0.083	0.154
62	151	0.030	(-0.109, 0.169)	0.180	0.166
63	130	-0.133	(-0.294, 0.027)	0.400	-0.333
64	104	0.012	(-0.194, 0.217)	0.476	0.024
65	104	-0.091	(-0.298, 0.117)	0.591	-0.154
66	96	-0.148	(-0.345, 0.049)	0.789	-0.188
55-60	829	-0.020	(-0.061, 0.021)	0.101	-0.196
61-66	721	-0.053	(-0.126, 0.020)	0.385	-0.138
55-66	1550	-0.036	(-0.079, 0.006)	0.247	-0.148

Difference calculated as treated average minus control average. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in their last observation. The average difference for age ranges (i.e., 55-60) is calculated by estimating separate differences for each age cohort and taking the mean of the estimates. Confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 7: Difference in Social Security receipt rate in last observation in 2008 panel between treatment and control group by parent's age

Dep. variable: Social Security receipt in last observation in 2008 panel			
	(1)	(2)	(3)
	<i>Parent's age in last observation in 2008 panel</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Treat (23-25 in 2010)	-0.0205 (0.019)	-0.0114 (0.019)	-0.0355 (0.034)
Monthly earnings (2009)	-0.0000136*** (0.000)	-0.00000890*** (0.000)	-0.0000159*** (0.000)
Paid job all of 2009	-0.0988*** (0.025)	-0.0805*** (0.027)	-0.128*** (0.042)
Female	-0.0179 (0.021)	-0.0418* (0.024)	0.00573 (0.034)
White (race)	0.0158 (0.024)	-0.00657 (0.028)	0.0391 (0.042)
Hispanic (ethn.)	0.0177 (0.042)	-0.0455 (0.046)	0.101 (0.075)
Married	-0.0336 (0.031)	-0.0198 (0.034)	-0.0491 (0.055)
HS or higher	0.0371 (0.039)	0.00724 (0.045)	0.0786 (0.077)
College or higher	-0.0519*** (0.019)	-0.0196 (0.020)	-0.0870*** (0.033)
Priv. HI all of 2009	-0.0794*** (0.028)	-0.126*** (0.034)	-0.0286 (0.048)
Num. children	0.00491 (0.009)	0.00652 (0.011)	0.00323 (0.015)
Age FE	X	X	X
Observations	1550	829	721
Control avg.	0.247	0.101	0.385

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in their last observation. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 8: Effect of treatment on Social Security receipt rate in last observation in 2008 panel, with age fixed effect

Dep. variable: retired in last observation in 2008 panel			
	(1)	(2)	(3)
	<i>Parent's birth year</i>		
	<i>1947-1958</i>	<i>1947-1952</i>	<i>1953-1958</i>
Treat (23-25 in 2010)	-0.0326* (0.018)	-0.0741** (0.034)	0.00148 (0.017)
Monthly earnings (2009)	-0.00000930*** (0.000)	-0.0000113*** (0.000)	-0.00000279 (0.000)
Paid job all of 2009	-0.196*** (0.026)	-0.287*** (0.045)	-0.123*** (0.029)
Female	-0.00148 (0.021)	-0.0134 (0.037)	0.0217 (0.021)
White (race)	0.0142 (0.023)	0.0443 (0.043)	-0.00598 (0.021)
Hispanic (ethn.)	-0.0335 (0.036)	-0.00215 (0.072)	-0.0498* (0.030)
Married	0.144*** (0.030)	0.135** (0.056)	0.145*** (0.029)
HS or higher	0.0175 (0.031)	-0.00948 (0.067)	0.0182 (0.030)
College or higher	0.00148 (0.020)	0.0214 (0.036)	-0.0132 (0.020)
Priv. HI all of 2009	0.0508** (0.025)	0.0791 (0.050)	0.0229 (0.021)
Num. children	-0.0164** (0.008)	-0.0179 (0.015)	-0.0139** (0.007)
Birth Year FE	X	X	X
Observations	1552	715	837
Control avg.	0.210	0.340	0.077

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes birth year fixed effect. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and were born between 1947 and 1958. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 9: Effect of treatment on retirement rate in last observation in 2008 panel, with birth year fixed effect

Dep. variable: Social Security receipt in last observation in 2008 panel			
	(1)	(2)	(3)
	<i>Parent's birth year</i>		
	<i>1947-1958</i>	<i>1947-1952</i>	<i>1953-1958</i>
Treat (23-25 in 2010)	-0.0187 (0.019)	-0.0323 (0.034)	-0.0111 (0.020)
Monthly earnings (2009)	-0.0000117*** (0.000)	-0.0000123*** (0.000)	-0.00000951*** (0.000)
Paid job all of 2009	-0.113*** (0.025)	-0.134*** (0.044)	-0.100*** (0.030)
Female	-0.0217 (0.021)	0.00973 (0.037)	-0.0528** (0.026)
White (race)	0.0200 (0.026)	0.0386 (0.044)	0.00112 (0.028)
Hispanic (ethn.)	0.0339 (0.043)	0.0954 (0.078)	-0.0199 (0.049)
Married	-0.0291 (0.030)	-0.0527 (0.055)	-0.00858 (0.034)
HS or higher	0.0514 (0.040)	0.126 (0.077)	-0.00152 (0.046)
College or higher	-0.0455** (0.020)	-0.0827** (0.035)	-0.0126 (0.020)
Priv. HI all of 2009	-0.0897*** (0.028)	-0.0493 (0.052)	-0.124*** (0.034)
Num. children	0.00884 (0.010)	0.0108 (0.015)	0.00565 (0.012)
Birth Year FE	X	X	X
Observations	1552	715	837
Control avg.	0.253	0.399	0.103

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes birth year fixed effect. Treated is defined as having a child aged 23-25 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and were born between 1947 and 1958. Standard errors are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 10: Effect of treatment on Social Security receipt rate in last observation in 2008 panel, with birth year fixed effect

Dep. variable: retired in last observation in panel			
	(1)	(2)	(3)
	<i>Parent's age in last observation in panel</i>		
	<i>55-66</i>	<i>55-60</i>	<i>61-66</i>
Treat full (20-22 in 2010)	-0.0241 (0.019)	0.00568 (0.018)	-0.0821** (0.038)
Monthly earnings (2009)	-0.00000488* (0.000)	-0.000000818 (0.000)	-0.0000130** (0.000)
Paid job all of 2009	-0.192*** (0.027)	-0.116*** (0.030)	-0.280*** (0.049)
Female	0.0134 (0.022)	0.0127 (0.025)	0.0284 (0.038)
White (race)	0.0578** (0.024)	0.0429** (0.019)	0.0714 (0.048)
Hispanic (ethn.)	-0.0191 (0.041)	0.00974 (0.040)	-0.0626 (0.086)
Married	0.143*** (0.031)	0.111*** (0.031)	0.171*** (0.065)
HS or higher	0.00440 (0.038)	0.0682* (0.035)	-0.127 (0.084)
College or higher	0.00946 (0.020)	-0.0286 (0.020)	0.0563 (0.037)
Priv. HI all of 2009	0.0404 (0.026)	-0.00581 (0.025)	0.125** (0.053)
Num. children	-0.0165** (0.008)	-0.0101 (0.007)	-0.0177 (0.015)
Age FE	X	X	X
Observations	1393	792	601
Control avg.	0.208	0.0788	0.331

*: $p < 0.1$, **: $p < 0.05$, ***: $p < 0.01$, based on bootstrap standard errors (in parentheses) and asymptotic normal approximation. Regression includes age fixed effect. Fully treated is defined as having a child aged 20-22 in 2010, and control is defined as having a child aged 27-29 in 2010. Paid job all of 2009 and private health insurance all of 2009 are dummy variables equal to 1 if the individual held a paid job every month in 2009 or had private health insurance every month in 2009. Sample restricted to individuals who exit the panel on or after September 2011 and are aged 55-66 in their last observation. Confidence intervals are estimated from non-parametric bootstrap sampling ($N = 1000$) with replacement from the initial sample, clustered by household.

Table 11: Effect of full treatment on retirement rate in last observation in 2008 panel, with age fixed effect

A Robustness Checks and Alternate Specifications

I consider a series of alternate specifications to confirm the robustness of the results. First, I use an alternate definition of cohort – birth year. The main results define cohorts by the age of an individual in their last observation in the panel, but an alternative way to define cohorts would be by birth year. These results will not exactly mirror those of the age cohort definition because each individual’s last observation in the panel differs, so two individuals born in the same year may exit the panel at different ages.¹⁶ If we define cohorts by age, then the gap in retirement rates represents the difference in the percent of individuals who are retired by a given age. Defining cohorts by birth year changes the interpretation of the difference to be the difference in percent of individuals in the same birth cohort who are retired by the time they exit the panel. The differences by birth year are plotted in Figure 10 and regression results are reported in Tables 9 and 10. The results by birth year are similar to the results by age, with older individuals driving a gap in retirement and Social Security receipt between the two groups. For parents born in 1947-1958 (who would be 55-66 in 2013), the treated group is on average 3.3 percentage points (15.7 percent) less likely to be retired. The difference is larger for older parents born in 1947-1952, where the treated group is 7.4 percentage points (21.8 percent) less likely to be retired. For Social Security receipt, the coefficients are again statistically insignificant.

I next consider alternate specifications with different definitions of treatment. In the main results, the treated group is defined as parents of 23-25 year olds. Because the dependent mandate only applies up until age 26, by the end of the panel in 2013, the children of treated parents were no longer eligible for the mandate. Since treated parents are no longer eligible for the dependent mandate in the post-policy period, we would expect that the results of this comparison would underestimate the results as these parents are only “partially treated” as their children are eligible for dependent insurance for only a portion of the post-policy period before 2013. An alternate specification would be to compare “fully treated” parents, or parents whose children are eligible for dependent insurance through 2013, to control parents. Fully treated parents are defined as parents of children who were 20-22 in 2010 and 23-25 in 2013, making them eligible for the

¹⁶About 90% of individuals in the sample leave the sample in 2013, meaning that most of the time when two individuals in the same birth cohort exit at different ages, the ages at which they exit will be at most one year apart from each other.

dependent mandate during the entire post-policy period. However, the age gap between children of control parents and children of fully treated parents is at least 5 and as high as 9, potentially invalidating the identification assumption that the two groups of parents would have similar retirement rates absent the dependent mandate. Thus, the preferred specification consists of comparing “partially treated” parents to control. The differences by age in the post-period are plotted in Figure 11 and regression results are reported in Table 11. We see that the results in the post-period are similar in magnitude and direction to the main results.

Finally, I consider alternate specifications that widen the bandwidth for defining treatment and control. The main analysis uses a bandwidth of 3 years, but alternatively I could have chosen to compare different-sized bandwidths. To see whether this affects the results, I define treatment and control with varying bandwidths from 1 to 5 years. A one-year bandwidth would compare parents of 24 year olds in 2010 (treated) to parents of 27 year olds (control), and a five-year bandwidth would compare parents of 20-25 year olds to parents of 27-32 year olds. The regression coefficients of treatment for varying bandwidths is plotted in Figure 12; the results are relatively stable across bandwidths.

B Data Appendix

The data used for this analysis are waves 1-16 of the 2008 SIPP panel and 1-12 of the 2004 SIPP Panel, which can be downloaded from [NBER](#) (last accessed April 4, 2020). Specifically, I use the core files for each wave, as well as Topical Module 2 (Fertility History) and Topical Module 6 (Employer-Provided Health Benefits). The .do file to extract, merge, and clean the SIPP files is named `sipp_clean.do`.

I exclude observations with interview status equal to “noninterview,” who are missing a sample unit identifier, and who are listed as men but have a non-missing value for the year they gave birth to their last child (I take this to mean that either the sex or the birth year of their child is coded incorrectly).

I define an individual to be retired if their employment status (*rmesr*) is listed as “No job all month, no time on layoff and no time looking for work” and the main reason for not working in the reference period (*ersnowrk*) is that they are retired. I define an individual to be receiving Social Security using the dummy for receipt of Social Security payments for self (*essself*).

Ages For each mother, I collect the year of her last birth (*tlbirtyr*) if she had more than one birth and the year of her first (and only) birth if she had only one birth (*tfbirthyr*). For mothers who are married, I assign to their current husband the same value for child’s birth year. For parent’s ages, I define age at last observation in panel using birth year and birth month to calculate age in months, and then round to the closest integer.

Treatment, control, placebo, and robustness The treated group is defined as parents whose children’s births were between 1985 and 1987, meaning their children were 23-25 in 2010. The control group is defined as parents whose children’s births were between 1981 and 1983, meaning their children were 27-29 in 2010. *Treat* is a dummy which is 1 for individuals in the treated group and 0 for individuals in the control group. The first placebo selects parents who were aged 55-66 in September 2010 and defines treatment and control the same way as the main results. The second placebo compares parents of children who were 31-33 in 2010 (born between 1977-1979) and 0 for parents of 27-29 year olds in 2010. The third placebo test defines treatment as having a child aged 23-25 in 2004 (born 1979-1981) and control as having a child aged

27-29 in 2004 (born 1975-1977). “Fully treated” is defined as parents whose children’s births who where 20-22 in 2010 (born 1988-1990). Bandwidth of 1 compares parents of 25 year olds to 27 in 2010, bandwidth of 2 compares parents of children aged 24-25 to 27-28, bandwidth of 3 compares parents of children aged 23-25 to 27-29 (main results), bandwidth of 4 compares parents of children aged 22-25 to 27-30, and bandwidth of 5 compares parents of children aged 21-25 to 27-31.

Other variables White is a dummy for whether an individual is white, defined using the variable *erace*. Female is a dummy for whether an individual is female, defined using the variable *esex*. Hispanic is a dummy for whether an individual is Hispanic, Spanish, Latino, defined using the variable *eorigin*. Married is defined as being never married, widowed, divorced, or separated and is defined using the variable *ems*. High school is defined as having *eeducate* being greater than or equal to 39, and college is a dummy for having *eeducate* greater than or equal to 41. Private HI in 2009 is a dummy variable for having private health insurance for at least half of 2009, defined by taking the average of *ehimth* in 2009. Employed in 2009 is a dummy for working a paid job for all of 2009, defined by taking the average of *epdjbthn* for all of 2009. Age in 2009 is the difference between 2009 and birth year. Number of children is defined using *tfrchl* for men and *tmomchl* for women, and is set as 0 if missing. Monthly income in 2009 is defined